Except for an unessential disturbance due to friction, the pressure all through the tube is uniform so far as the wind-motion is concerned.

Erect across the tube a couple of imaginary partitions, and watch the substance streaming past them. The state of the substance at either partition, whatever it may be at one instant, remains permanently the same always; hence the mass of substance inclosed between the two must remain permanently the same—for it cannot be always steadily increasing—and therefore *the mass of matter flowing through any one plane is constant.*

In future attend to one of the planes only, and call the density of the substance at this place $\rho$. The plane may be at or near a condensation, it may be at or near a rarefaction, or, again, it may be where the substance has its ordinary density; whatever the state of the substance there, the same it remains. The longitudinal-pulse motion of the particles of substance (which has previously been illustrated and discussed at length) is superposed upon the wind-motion; and if we followed any one particle along the stream we should see it simply oscillating with a simple harmonic motion—

$$x = a \sin nt, \quad \text{or} \quad v = an \cos nt,$$

having at any instant the velocity $v$.

But we are not going to follow a single particle down stream; we are contemplating a procession of particles as they successively pass the fixed partition, and at the instant of passage they are all in a definite phase of their motion—they all have the same definite velocity, $v$, as they pass, in addition to their general wind-velocity, U. The vibratory velocity $v$ may be in the same direction as U, or it may be in the opposite direction; it may have any value between $\pm na$, of course. So the resultant velocity of each particle as it passes the fixed partition is algebraically $U + v$. This represents the length of cylinder of substance passing through the partition per second; and, since the partition is of unit area, the mass of substance flowing past it per second is

$$m = (U + v)\rho, \text{ and is constant } . \ . \ . \ . \quad (1)$$

This of itself is an interesting result; for it shows that at the middle of condensations, where $\rho$ is a maximum, $v$ must have its greatest negative value; and the particles are therefore all in full swing back against the wind (*i.e.* travelling with the sound-pulse) at the middle of every condensation. At a rarefaction, $v$ has its greatest positive value, and the particles are swinging with the wind (against the sound-pulse). Only at half-way places, where the density of the substance has its average or undisturbed value, are the particles quiescent as regards the sound-pulse.

Next consider the dynamics of the matter, and the force which must act to vary the motion of the particles.

If the pressure were the same on either side the partition, there could be no change of velocity for the particles as they pass. The charge of velocity, $d(U + v)$, or $dv$, must be due to a difference of pressure existing on either side the partition; and if the slope of pressure $\frac{dp}{dx}$ is positive, the pressure is greater on the lee-side of the partition than on the windward side, and so the acceleration $\frac{dv}{dt}$ will be negative. Hence, equating the force acting and the momentum generated by it per second,—

$$dp = - m dv \ . \ . \ . \ . \ . \ . \ . \quad (2)$$

This equation, along with equation (1), solves the problem, and determines the velocity U.

Differentiating (1)—

$$(U + v)d\rho + \rho dv = 0.$$

Rewriting (2) by help of (1)—

$$(U + v)\rho dv = - dp.$$

Substituting for $\rho av$ from one of these into the other, we get—

$$(U + v)^2 = \frac{dp}{d\rho} \ . \ . \ . \ . \ . \ . \ . \quad (3)$$

This equation shows that $\frac{dp}{d\rho}$ is by no means constant all through the substance. It is greatest wherever $v$ has its maximum positive value—that is, at the centre of every condensation; it is least at the centre of every rarefaction; it has an average value in the undisturbed portions of the medium, and it is there equal to $U^2$.

Hence we learn that the value of U is determined by calculating the ordinary value of $\frac{dp}{d\rho}$ for the medium in its uncompressed and unrarefied state.

So—

$$U = \sqrt{\frac{dp}{d\rho}} \ . \ . \ . \ . \ . \ . \ . \ . \ . \ . \quad (4)$$

This is the velocity with which the substance must flow *through the tube in order to keep the sound-pulse stationary*: this, therefore, is the velocity of sound in it.

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The result is general, and applies to all substances. But for gases it may be written more explicitly by help of their characteristic equation $\frac{p}{\rho} = RT$, and their adiabatic condition $p \propto \rho^\gamma$: viz.—

$$U = \sqrt{\gamma RT} \ . \ . \ . \ . \ . \ . \ . \ . \ . \quad (5)$$

T here means the undisturbed temperature of the gas, but if one chooses to allow the equation to follow the fluctuations of temperature ($\pm t$) adiabatically produced in the condensations and rarefactions, one must write the more general form—

$$(U + v)^2 = kR(T \pm t) \ . \ . \ . \ . \ . \ . \quad (6)$$

which gives us the relation between fluctuation of temperature and vibrational velocity. We may also write it—

$$\rho^2(T \pm t) = \text{const.} \ . \ . \ . \ . \ . \ . \ . \ . \quad (7)$$

which shows the connection between the elevation or depression of temperature, and the density, at any part of a sound-wave.

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Speaking as a teacher, I believe one reason why we fail to make things clear is, because we are often in too big a hurry. One's natural tendency is to give such an investigation—as this, for instance, in Maxwell's "Heat"—in a few lines on the blackboard, taking perhaps half an hour or less over it, and forgetting that it embodies in concentrated form a great deal of difficult thought, though the actual mathematics may be simple. Gradually I am learning not thus to scamper over the ground, but to lead up to a thing in two or three or even more lectures, and then to devote a whole hour to the thing itself. By this means, students may ultimately be got to grip and feel the thing as a whole, instead of having to ascend step by step to it; but it is hopeless to expect them to thus grasp it straight off; and even if it were possible, it would not be really desirable for various reasons. The attempt to hurry them into the comprehension of difficulties leads them, I believe, into a vague notion that everything is hazy and half unintelligible. The best thing we can do for them is to get them to see some few things luminously, so that they may not feel inclined to rest satisfied with half-knowledge in other instances.

OLIVER J. LODGE.

November 12.

P.S.—Since writing the above, I have referred to Prof. Everett's note A in Deschanel, and have found it excellent, like all his notes; he happens to have employed just the same means as the above for obtaining equation (1), but for the latter part I prefer my statement. I trust no one will imagine that the above contains anything more than a way of putting things to students.

The slip of a wrong sign in Maxwell I had not distinctly noticed, but the simplest statement of it seems to be that in obtaining the second equation he has put, for the change of velocity of each particle as it passes a plane, $du$ instead of $d(U - u)$; that is, the change of absolute instead of relative velocity.—O. J. L.

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## A Simple Dynamo.

I VENTURE to send you a brief description of a simple electromagnetic instrument which I have recently devised for illustrating the principle of the Gramme ring.

Two pulleys, A, B, having semicircular grooves, are mounted, as shown in the figure, on a piece of board; round the two wheels is stretched a continuous coil of copper wire; a horse-shoe magnet is placed with its poles close to the vertical parts of the coil; the wheels are connected to the terminals $t$ $t'$: when the wheel A is rotated the whole coil moves, and a steady current is at once generated, which flows from terminal to terminal when they are

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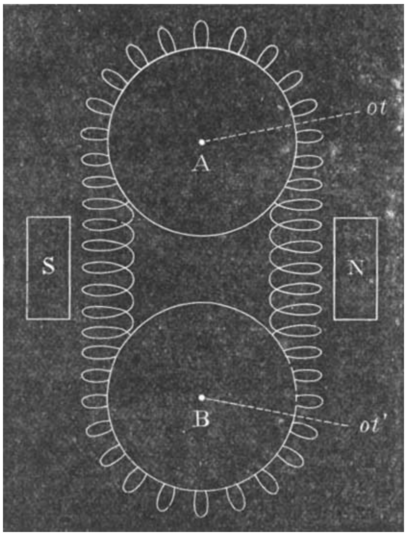
connected together, the direction of the current depending on the direction of rotation.

It will be noticed that, since the coil and wheels are always in contact, no undulations are produced as when brushes come in contact with successive ends of coils, as in the ordinary dynamo.

When the instrument is placed in circuit with a sensitive galvanometer, the rotation being constant, no variation in the current can be detected, even when the motion is very slow. The coil when arranged with one wheel at A, and a mercury contact at B, will revolve when a current is sent through it, becoming in this case a motor. If an iron chain, or an elastic band of iron such as a measuring tape, be placed inside the ring coil, it then becomes a distorted Gramme ring, the wheels taking the place of the brushes, the way in which the current is produced being the same. If coils approaching N produce a current upwards, then those which are leaving N produce one downwards. The same takes place on the other side; coils leaving the S pole produce a current upwards, while those which approach it produce a current downwards; both of the ascending currents, being in the same direction, go to the wheel A, while both of the descending currents, being in an opposite direction, go to the wheel B.



The first coil made was of copper wire. Phosphor bronze wire answers better, being less easily distorted.

Frederick J. Smith.

Trinity College, Oxford, November 12.

### The Use of Rotifera.

Can any of your readers inform me whether Rotifers are of any use in removing decaying organic matter, as Infusoria do?

C. L.

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## *ON THE MECHANICAL CONDITIONS OF A SWARM OF METEORITES.*[1]

### I.

Mr. LOCKYER writes in his interesting paper on meteorites[2] as follows:—

"The brighter lines in spiral nebulæ, and in those in which a rotation has been set up, are in all probability due to streams of meteorites with irregular motions out of the main streams, in which the collisions would be almost *nil*. It has already been suggested by Prof. G. Darwin (Nature, vol. xxxi. p. 25)—using the gaseous hypothesis—that in such nebulæ 'the great mass of the gas is non-luminous, the luminosity being an evidence of condensation along lines of low velocity according to a well-known hydrodynamical law. From this point of view the visible nebula may be regarded as a luminous diagram of its own stream-lines.'"

The whole of Mr. Lockyer's paper, and especially this passage in it, leads me to make a suggestion for the reconciliation of two apparently divergent theories of the origin of planetary systems.

The nebular hypothesis depends essentially on the idea that the primitive nebula is a rotating mass of fluid, which at successive epochs becomes unstable from excess of rotation, and sheds a ring from the equatorial region.

The researches of Roche[1] (apparently but little known in this country) have imparted to this theory a precision which was wanting in Laplace's original exposition, and have rendered the explanation of the origin of the planets more perfect.

But notwithstanding the high probability that some theory of the kind is true, the acceptance of the nebular hypothesis presents great difficulties.

Sir William Thomson long ago expressed to me his opinion that the most probable origin of the planets was through a gradual accretion of meteoric matter, and the researches of Mr. Lockyer afford actual evidence in favour of the abundancy of meteorites in space.

But the very essence of the nebular hypothesis is the conception of fluid pressure, since without it the idea of a figure of equilibrium becomes inapplicable. Now, at first sight, the meteoric condition of matter seems absolutely inconsistent with a fluid pressure exercised by one part of the system on another. We thus seem driven either to the absolute rejection of the nebular hypothesis, or to deny that the meteoric condition was the immediate antecedent of the sun and planets. M. Faye has taken the former course, and accepts as a necessary consequence the formulation of a succession of events quite different from that of the nebular hypothesis. I cannot myself find that his theory is an improvement on that of Laplace, except in regard to the adoption of meteorites, for he has lost the conception of the figure of equilibrium of a rotating mass of fluid.

The object of this paper is to point out that by a certain interpretation of the meteoric theory we may obtain a reconciliation of these two orders of ideas, and may hold that the origin of stellar and planetary systems is meteoric, whilst retaining the conception of fluid pressure.

According to the kinetic theory of gases, fluid pressure is the average result of the impacts of molecules. If we imagine the molecules magnified until of the size of meteorites, their impacts will still, on a coarser scale, give a quasi-fluid pressure. I suggest, then, that the fluid pressure essential to the nebular hypothesis is in fact the resultant of countless impacts of meteorites.

The problems of hydrodynamics could hardly be attacked with success, if we were forced to start from the beginning and to consider the cannonade of molecules. But when once satisfied that the kinetic theory will give us a gas, which, in a space containing some millions of molecules, obeys all the laws of an ideal non-molecular gas filling all space, we may put the molecules out of sight and treat the gas as a plenum.

In the same way the difficulty of tracing the impacts of meteorites in detail is insuperable, but if we can find that such impacts give rise to a quasi-fluid pressure on a large scale, we may be able to trace out many results by treating an ideal plenum. Laplace's hypothesis implies such a plenum, and it is here maintained that this plenum is merely the idealization of the impacts of meteorites.

As a bare suggestion, this view is worth but little, for its acceptance or rejection must turn entirely on numerical values, which can only be obtained by the consideration

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[1] Abstract of a Paper read before the Royal Society on November 15 by Prof. G. H. Darwin, F.R.S.

[2] Nature, November 17, 1887. The paper itself is in the Roy. Soc. Proc., November 15, 1887 (No. 259, p. 117).

[1] *Montpellier Acad. Sci. Mém.*

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